S. W. 1860 complete
Mercy

A ChoLOGUE. Anti-philosophic. and Anti-philosophic.

by

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"... assiduitate quotidiana et constantine peculiarum assecimunt animis, neque admirantur, neque requirunt rationes eorum rerum, quassesper evident, perinde quasi mortas nos magis quam magnituder rerum, debeat ad coquendias Causas excitare."

Cic. de Nat. Deorum.
Index

Subject
Introduction
Mecury as a Scholagogue
Mecury as an Antitiphilistic
Mecury as an Antiphlogistic
Conclusion

Page
1
5
18
37
62
Nothing is more remarkable in the history of medicine than the credit given to different curative agents, and the faith reposed in them, at different times. From our pharmacopoeias how many drugs, once infallible, are now utterly discarded. How many do we not still encounter, which have fallen into desuetude and neglect from being once lauded and praised. How many have had actions attributed to them, and have been given to produce these actions, which we now know they neither could have had nor could have produced. Every now and again some specific is started, tried, and thought successful, for some time continues to be a favourite and is extensively employed, then forgotten to be again, it may be, thought for by its obscurity at some future period again to run the same course. Especially has this been the case in those more obscure diseases, with which, either as to cause or nature, we are but little acquainted. How many specifics have we not had for Cancer. How many infallible remedies for Consumption, and that not merely from the Charlatan or impostor. Yet still these maladies are in many cases incurable, as ever defying alike our skill and our remedies, and ever and anon attacked by new drugs, which after a brief existence are forgotten, these diseases in general being too certain in their result and too convincingly intractable for any drug to maintain long a reputation for their cure. It is in those not necessarily, not commonly fatal - those that in themselves recover...
recover, that drugs have flourished most luxuriantly in reputation and that we have led the most of the poet hoc ego profiteur hoc. And only now, that we are inclined to trust a little more to nature and her wise medicaments, have we learned that the former reputation of many drugs in particular diseases is fallacious and doubtful. We now know that the tendency of many diseases at least is to health, and that they will run their own course without treatment at all; a fact not sufficiently known or appreciated by many of the older physicians, who seem to have looked on the tendency of every disease as being to death, and consequently launched against the whole force of medicine. But we know that some diseases recover under the most opposite treatment as well as under no treatment at all; and hence we can trace the many different properties assigned to many different drugs, as well as the grounds for the alleged virtues of others. Nor can deny that our therapeutics are to a great extent Empirical. In most cases we give a medicine because we have read or heard that, in the hands of others, it had produced results such as in a given case we wish to produce. As to the modus operandi in most cases we know little or nothing. Nor is it at all surprising, considering how fallacious experience is, how proteins-like is disease in its forms and how varying in its terminations, that many mistakes as to the use of remedies should have been made. And that many have been made we have only to look into a work on Materia Medica of the past and compare it.
it with one of the present, to compare the reputation with the observed actions of medicines, or the past with the present treatment of disease. Now different, for example, is the treatment of disease as laid down in most text-books from that pursued in our hospitals! We see drugs recommended, may commanded in certain diseases; but we never see them used. We see it stated that only by a certain treatment can a certain disease be successfully combated, yet we find these diseases recovering in many cases under a treatment diametrically opposite, and in others under none at all. In no case is this contradiction between precept and practice more observable than in the use of blood-letting and Ng. How often do we find the former recommended in our standard works, yet we have never seen it prescribed or ordered in our Hospital practice. In how many diseases is Ng. to be given as the only means of curing, yet I have very seldom indeed ever seen it prescribed in Hospital practice? Indeed for the last three months, it has never been prescribed in our Clinical Ward. Why is this? Are these remedies once thought so potent, no longer so? Or was their employment a mistake and their reputed virtue an error?

Of blood-letting we do not intend to treat, but we shall endeavour to ascertain if Ng. is actually such a valuable remedy as many suppose it to be, and if it deserves the high reputation which it has enjoyed and still enjoys with many does enjoy.
a drug, so famous and so well established, could ever be called in question, and for less fall into desuetude or neglect. It had still been reserved, when many others had been expunged. Its actions and uses seemed manifold—its virtues legion, and it seemed to bid fair to remain the Alpha and Omega of the pharmacopoeia—the greatest and most trustworthy of all remedial means for the cure of all the ills that flesh is heir to. But how different now! Some deny, many doubt, but none use it to any thing like the extent that it once was used. Is this well? Are these sceptics right? Are these doubts well founded? If this modified use or this total neglect consistent with the well-being of the patient? Can disease, in short, be combated as well without as with Mercury?

Prof. Christianson says there is no other substance in nature which excites so great a variety of actions on the human body as Mercury. Physiologically it acts as a Corrosive. An irritant, An Astringent, An Ocllante, A Cholagogue, An Emmenagogue, A Diaphoretic, A diuretic, A Cathartic, A Stimulant, and a Cholagogue, therapeutically as an Antiphlogistic, an Antispil-

atic, An Anthelmintic, A Deobstinent, An Alternative, and a Sedative— In all seventeen reputed actions on the human body. If it actually possesses all these virtues—if it actually does all that it is said to do—we may well ask, what does it not do, and what the use of so many different drugs, since we have in one all the virtues of all. But does it actually possess all the
all the properties assigned to it? We have no time to discuss all its reputed actions, but to those only of the most important of these would we direct our attention viz. its physiological action as a Chologogue and its therapeutic actions as an Antisyphilitic and Antiphlogistic.

1st. Mercury as a Chologogue.

Its action of Hg. has been and is more insisted on than its action on the Liver; and certainly in hepatic diseases it has been used most profusely and with apparently the most diverse intentions. When the biliary secretion was supposed to be defective, it was given; when excessive, still it was given; when the Liver was inflamed, Hg. was the remedy most trusted to; when hypertrophied, it was exhibited; and when atrophied, it was Hg. still. This is no exaggerated statement of its use at one time at all events, and even yet in many cases it seems not far from the truth. Of the pathology of hepatic diseases — of the physiology of the Liver and its secretion — the bile, but little is known, and certainly it would be most convenient had we one uniform and unfailing remedy for all its diseases however different, and for all its states and conditions.

Have we such a drug in Mercury? May we not doubt some at least of the actions attributed to it? If it is a remedy when the hepatic secretion is diminished, how can it be so when that same secretion
is increased? If it can reduce a hypertrophied Liver, how can it enlare one that is atrophied? If it can soothe when excited, how can it prove when depressed? Yet we find all these different actions attributed to it, and for all these different purposes as we find it given. And, strange to say, in all these diverse cases the modus operandi of the cure is said to be the same. It is accomplished in all alike by its power or supposed power of stimulating the Liver to increased functional activity, and thus producing an increased secretion and discharge of bile. This seems to have been one of the first uses to which it was put. Petrius recommends it ad viscidos humorum magis attenuandos." Schroeder says "Mercurius dulcis vulgaris, draco mitigatus, omnes humorum moxios sine perturbatione lunde expurgat; and so on; the idea apparently being that it possessed a power over all the secretions and excretion of the body, but especially over the biliary; and this idea seems to have been adopted almost unquestioned in the present day. Thus Annecley, in 1828, reproving the "indolence in research" which received in unquestioning and undoubting faith the teaching of the past, published some experiments, made by him on the effect of large doses of Mercury; and though these experiments were not directly instituted to test the Chologogue action of that drug, yet he seems to have been satisfied of its specific power over the Liver. For he speaks of an immediate and overpowering
overpowering flow of bile as often following a single dose of the medicine - a statement which, so far as I can see, his experiments do not prove. Farnia says, mercurials cause an increased activity in the secreting and excreting apparatus, the quantity of intestinal mucus of bile &c being augmented. Again he says, mercurials promote the secretion from the mucous follicles of the intestines from the Liver and Pancreas.

Meligan says, Colonel is often used in consequence of its action on the secreting organs, stimulating the Liver and intestinal glands to increased activity, and it is therefore peculiarly adapted for all diseases attended with functional derangement of the hepatic system. Budd says, of medicines that render the secretion of the Liver more active and thus increase the flow of bile, or, as they have been termed, Cholagogues, the most energetic is mercury. Prof. Christiano says, Mercury unquestionably possesses the property of stimulating various secreting glands - of raising the Liver to an increased discharge of bile without necessarily inducing its peculiar Constitutional effect. Again he says, speaking of the Cathartic Action of Mercurials, their action on the bowels is believed to be always attended with an increased discharge of bile from the gall bladder.

Such then, are some of the opinions expressed as to this specific action over the Liver. On what grounds are these opinions founded? There are no direct experiments
experiments given to support them, and they seem to rest chiefly on the results of its use in hepatic disease or disorder and secondly on the state of the stools as proving that it did act on the liver and excite an increased flow of bile. As to the first, many cases of all curable diseases of the liver recover without the use of mercury and many recover under its influence, while some cases recover without any medicine whatever being given. Take jaundice for example. The stools are clay-coloured, the skin is icteric and all the usual symptoms present. You give mercury, because there is a deficiency of bile excited by the liver (supporting that the theory of the case formed) and by and by the patient recovers. The natural inference is, that the mercury, through stimulating the sluggish liver and thus restoring the normal amount of bile, has cured. But take a similar case and treat it differently: say with bluebark, diaphoretics and diuretics. Though length of time the conjunctiva becomes clear, the skin loses its yellow tint and health is restored as well and as quickly in the one case as in the other. What is then the natural inference? Why, that if mercury can cure icterus, so can bluebark, or the many other remedies that might have produced the same result, if the one has a specific action, so have the others; and thus it is with all the curable diseases incident to the liver. In all mercury has often been employed without success, where others succeeded; and it has succeeded.
succeeded, where others failed. It is not, therefore, an infallible remedy, and in diseases of the liver possesses no virtue of which other remedies might not claim a share. But taking it for granted that it has the power alleged, and can stimulate the hepatic organ, what is the rationale of its exhibition? We are notoriously ignorant as to many of the morbid states of the liver. Or very few, if any, can our diagnosis be confidently trusted to. What indications have we for our guidance in its exhibition? Surely in many cases that same stimulant property must do harm. Thus, suppose the secretion of bile normal, but from some cause its discharge prevented. Can anything but harm follow the stimulation of the organ? The blood is uncharged with bile absorbed from non-excretion (we are supposing the liver acting normally, and when can we say it is not) is it the proper method of removal to stimulate to increased secretion? In the treatment of inflammation of other parts, the first indication is rest of the affected part. In what does inflammation of the liver differ, that we should seek to stimulate it and it alone when inflamed? In hypertrophy proper of the liver? Can we expect to reduce by exciting increased action? Does Atrophy of the liver ever depend on diminution of functional activity? Perhaps, that we would seek to remedy it by striving to rouse to increased activity? Should not these cases rather Contra-indicate its use? Yet in all it is given
given, and apparently with success in some cases. Can its action then, be such as represented? Is not its use empirical? With many is not a diagnosis of hepatic disorder of whatever kind at once an indication for its exhibition? Is it not abused and does it not often do harm? Budd says, "The marked temporary benefit resulting from mercury given to render the liver more active has, from the difficulty of distinguishing the various diseases of the liver and the consequent indiscriminate use of the drug, led to great evils. This medicine was at one time by English practitioners given almost indiscriminately and long persevered in for disorders of digestion, many of which did not depend on the fault of the liver at all, but on local disease of the stomach or intestines or on faulty assimilation, the result of debility, which the prolonged use of mercury but too often increased. Of late these evils have much abated, but still before the diagnosis is rightly made, mercury is often tried in Cancer and other incurable organic diseases in which it can only do harm."

Our knowledge of the pathology of the liver has advanced much within the last few years, but may our use of this drug not be even still too indiscriminate? Increasing knowledge has robbed it of half its virtue, may it not deprive it of the whole? Our means of diagnosis of hepatic diseases are far from perfect; our knowledge of the drug we administer...
administer is hypothetical and, as I shall hereafter prove, more than doubtful. Ought we not, then, to be more careful in its use, and does the employment of this drug in diseases of the Liver, various as they are and various as are the results it has seemed to produce, and do the effects of that employment and the unity of these results warrant a conclusion as to its stimulant action on the Liver? Do they not rather point to some other and different action, if to any action at all? From the manner in which it has been and is still used it would seem to concentrate in itself many properties differing widely as the diseases for which it is given, in their nature. Certainly its exhibition in disease does not prove its photogogue action.

The stimulating action of mercury on the Liver has been inferred from the appearance of the stools which are passed when it is given as a purgative or even otherwise. Thus the stools may be clay-colored and evidently deficient in bile; Mercury is given, they become more or less black or greenish; and that has been supposed to indicate a restoration of the normal flow of bile. Does it indeed prove this? Annually from direct experiments ascertained that the admixture of Calomel with the tenacious mucous secretion of the intestines produced a dark gray and pulvaceous compound, and that when bile was added to this a dark green and more fluid compound was formed, similar
similar, says he, to the character of the motions, when
the biliary evacuation is occasioned by this remedy.
Therefore, when we see the change from dark gray (the
colour which Colonel alone gives the mucous secretion)
to dark green, we may rest satisfied that the ducts
are emulsified, and that the Colonel and Cystic bile
are acting conjointly on the bowels; and in a note
he adds, but I question whether Colonel or bile or
both acting upon the natural and healthy secretion
of the bowels will produce any discoloration in the
motions. Now this proves that dark gray stools
after the exhibition of Mercury do not prove or
show its action on the Liver; and as to the dark
green stools I think his opinion may well be
disputed. I killed a healthy Cat by hydrocyanic
acid, and then immediately, opening it I mixed
Colonel with the thick mucus which coated the intestinal canal, and a dark colour
was speedily produced, certainly more nearly allied
to green than gray. I next mixed some Colonel
with the bile and a putrescent mass was formed
but no change in Colour took place except what
could be referred to the colouring matter of the bile
itself. I then poured some bile over the dark
compound already mentioned, and certainly the
colour became lighter, but no more so than the
dilution, modified by the colouring matter of the
bile, could explain. Certainly nothing like the
Spinach...
spinach stools so much insisted on as showing the characteristic action of mercury in children, was produced. From this experiment we would infer then, that neither the dark-coloured stools nor the greenish-coloured are to be referred to the admixture of bile; and also that Cazenove can produce the dis-colouration when mixed with the healthy secretion of the mucous memban, and that, therefore, these dis-colourations occurring after the use of mercury are not to be held as proving a morbid condition of the intestinal secretion. The dark colour of the stools we can easily account for; Can we as account for the green otherwise than through the action of bile? I think we can. In dysentery and in some diarrhoeas the stools are frequently green or black, and this is explained in these diseases through the action of the intestinal gases on the blood diffused into the intestinal canal. Now mercury is a violent pur-gative, especially when as is frequently the case, it is conjoined with other drugs of a drastic nature, and from its action blood is frequently seen in the stools, and this some experiments of Swain proved; the stools in the instances given being often of a colour more or less dark containing blood and that even when no mercury was given internally, it being only used as an ointment. He did not attribute the dark colour to the blood, but from his description of the appearance of the stools, the chocolate and coffee
and coffee colour mentioned by him, we can have little doubt that it was due to blood. The blood he describes must have been of its normal colour and had been effused from the lower end of the intestinal canal and had not had time to be acted on by the gases. Be that as it may, however, the fact is proved that blood was effused under the purgative action of mercury. And if we attribute the discoloration of the motions in dysentery to the intestinal gases, why may we not do so in mercurial purging? Those discolorations do not always take place—blood is not always effused; but rarely indeed can bile be entirely absent from the intestinal canal. The motions at first are dark—blood becomes effused through its severity and then the stools become green. This seems to me the most rational explanation of the results of mercury given to children in whom no hepatic derangement is ever thought of; and in whose intestines bile must be present, but whose stools in general do not show the green discoloration till some doses have been taken. Thus, then, in my opinion the character of the stools does not at all prove the action of mercuries on the liver, though certainly it does prove its action on the intestinal canal, and we have already shown that the mere recovery from hepatic complaints through its exhibition does not prove its stimulating action at all events and may with just as much force be held
beld to prove it a sedative, an astringent or anything whatever.

These opinions may seem hypothetical, and though it might be urged that they are no more so than the alleged virtues of the drug, we have something more certain still to adduce in favour of our views on this subject. In Beale's archives Vol. I we find a report of a series of experiments, instituted by Dr. Scott, to determine if the preparations of mercury really increased the flow of the bile, as was generally believed. Four experiments are detailed as well as the means taken to render them free from error or mistake and as far as I can see no objection can be urged against them, every possible source of mistake being most carefully and ingeniously guarded against. Surprising indeed, were the results considering how long the opposite opinion had prevailed. For these experiments prove indisputably that mercury in purgative doses instead of increasing the flow of bile actually diminishes it. Thus he says,

(and a reference to the table proves it) All these experiments with Calomel give one result, namely a diminution in the amount of bile and bile-stools.

Surely, then, these results and considerations call for some modification of the prevalent idea as to the Cholagogue action of this drug. It acts as a purgative that no one can deny. By its purgative properties it may (and other Cathartics may do the same) clear away
away some obstruction from the intestinal orifice of the ductus communis choledochus, and thus allow bile, pent up from such an obstruction, to escape; but that it exerts a specific stimulant action on the hepatic organ itself we may, I think, well doubt till better proof at any rate is afforded than we now possess. And it should not be forgotten that if it can do good, it can also do and has done incalculable evil.
Mercury as an Antisyphilitic.

Within the last few years nothing in medicine seemed more surely established than the power of mercury over the syphilis, or rather, the venereal disease, for which it was regarded as the only antidote—the only trustworthy means of cure. Since the days of Paracelsus down to the beginning of the present century we find it more or less largely and generally most profusely administered. But a revolution of opinion has taken place, and the dogmas of centuries in respect to it are now set at nought and looked on as pernicious and injurious, and by many its antisyphilitic power is wholly denied and its administration declared a monstrous mistake. We shall arrive at the truth, I believe, by contrasting the results of the treatment of syphilis when mercury was held to be the sole and sovereign remedy with the results now in the many cases in which it is never given at all.

Its use in the venereal disease is first noticed about the beginning of the 16th century, and it seems in a very short time to have been established as the sole reliable drug for the cure of that then much dreaded disease. And, truly, their faith in it must have been great indeed from the doses in which it was given and from the length of time its use was persisted in. Thus Weismann directs twenty or thirty grains of Calomel as a dose daily to be followed by
by a few grains of turpentine (metal), if, as he says, after three or four days their chinks do not swell. This is his mildest course. Sydenham directs the salivation to be so managed that the patient shall spit about four pints of saliva in the twenty-four hours, and this to be continued for three weeks or a month after all symptoms of the disease have disappeared.

Eberhane is more heroic still. He says, the fat of the body, even to the last particle, must be drawn off in the cure of the venereal disease; for if even the least particle be left, we must dread a relapse. He is content if three pints of saliva be discharged in the twenty-four hours. Dr. Hunter says, "Mercury in Lues venerea as in Chancre is the great specific. I have never," says he, "seen a case where neither external nor internal applications of mercury were not absorbed. Such a case must be miserable indeed. And to such an extent did Hunter carry his faith in the antisyphilitic power of mercury that he considered it actually diagnostic of the disease, and if in a given case it failed to cure, why, that case could not be syphilitic.

Now, however, we cannot help thinking that some, at least of the cases declared by him non-syphilitic on this ground were actually syphilitic though they did recover under tonics and general hygiene, after mercury had failed. It is only wonderful that from the different results of mercury in the treatment of Gonorrhoea and Syphilis—results which seem somewhat
to have surprised him; he did not attribute these
diseases to different poisons. In he says, "Mercury, so
powerful over Chancræ and Syphilis, which are in
no respects alike is powerless over gonorrhœa which
in many respects is similar to Chancræ." Mathias
writing on the mercurial disease, which he claims to
have been the first truly to discriminate and describe,
and fully alive to its terrible nature, yet says, "the
Lues Venerea can only be said to be incurable when
the disease has got to that deplorable point by ne-
glect that the emaciated hectic state of the patient
forbids its use." Cullen says, "The advice for the early and
full use of Mercury I take to be the most important
that can be given, and I am convinced that most of
the cases which have occurred of the violence and obstinacy
of Syphilis, have been owing nearly entirely to the re-
glect of the early applications of mercury." He too, like
most others, urged its administration as a preventative.

Such then, were the opinions that actuated men in
the treatment of Syphilis up till no very remote
date. What was the result? It is now acknowledged
on all hands to have been terrible indeed. Let
Sydenham describe it in his time; for we now know
that he describes the result of the remedy (the effects
of the four pints of saliva per day) whereas he fancied
that he only described the syphilitic disease itself when
mercury had failed to cure. "When," says he, "A
gonorrhœa has been protracted, or when from the
undue use of astringents a taint has been imparted to the blood. Lies arises, tubercles appear on the groin, there is pain in the head, limbs and joints; there are crusts and scurves on various parts of the body yellow like honey combs. In the pericranium, shin bones and arms there are extases, there is inflammation and caries of the bones. Phlegmatic ulcers on various parts of the body but most on the face; they spread gradually along the face and palate and destroy the cartilages of the nose which then falls in. The caries, ulcerations and pains increase limb by limb the patient drops off, and his lacerated body, a burthen on the earth, drops into the grave. Surely this picture is sufficiently terrible; but we know that it is not overdrawn, and that it was not an uncommon one in his day and long subsequent. Hunter, though often checked in its exhibition by his theory as to mercury being diagnostic, could have drawn many such and has given the outline of one. Dörerhauer could have equalled it, perhaps even surpassed it. The originals were not uncommon in those days, and many a "lacerated body" I have no doubt did he send to the grave in his terrible attempt to remove alike every particle of fat and every trace of poison. No wonder they dreaded the disease, to which alone they attributed results so terrible. It was the venereal virus and it alone that caused that awful catalogue of evils; and for it and for them mercury was the great antidote and the only certain cure. A man had
had contracted venereal disease or had rendered himself liable to it. Mercury was given to cure in the one case, and to prevent in the other. And probably the great majority of the cases got well in this stage. But, perhaps, they did not, and secondary symptoms appeared (and we know that then they were more common than now) an eruption appeared on the skin, (we are supposing it syphilis) ushered in by febrile symptoms, the throat ulcerated, the complexion changed, severe pains with nightly exacerbations, and other symptoms more or less grave set in. Then if the primary sore had not been treated by mercury, all the more use for it now; if it had been so treated, the occurrence of these secondary symptoms showed that it had not been given to an extent sufficient to neutralize completely the venereal poison. And mercury alone had that power. So in either case it was indicated and given so as to produce in many cases most "virious" salivation, which was kept up for a longer or shorter time, till under it the patient got thin and emaciated, and his Constitution broken and shattered. Still if the syphilitic symptoms disappeared, he was reckoned cured—saved by mercury; and probably still the greater number recovered, if a result so terrible could be called a cure. But suppose these symptoms did not disappear, but became aggravated, foul crusts and sores forming on the skin and extending rapidly by ulcerations, phagedena...
a sloughing with intractable ulcerations of throat, tongue, cheeks and gums. Exploitation of the hard palate and of the nasal bones. Ulceration of the nose, lips and face (causing horrible deformity). Nodes, ulcerations, caries and necrosis appearing in regular sequence in a constitution worn out and exhausted, why still it was Mercury. Mercury was the only means of averting a fatal issue and of giving a few miserable years of life to that 'scarfully disfigured body'; and of course it was still given—given freely, fully and fearlessly. And now few indeed did escape; most dropped off limb by limb into the grave. A few survived, hideous in their own eyes and disgusting in those of others, ever liable to hideous diseases which with their deformity must have made life more terrible than death. And still all that was attributed to the venereal virus and to it alone. Mercury could cover the body with eruptions, could cause ulcerations and sloughings in other diseases, but in the venereal disease its results were all benign. In it, it was potent only for good. Thus Hunter says, "it is nearly as dangerous in many constitutions to give mercury when the disease is not venereal as to omit it in those which are; for we may observe that many of the constitutions which put up some of the venereal symptoms when the disease is not present, are those with which mercury seldom agrees and to which it commonly does harm." I have seen," he continues, "mercury given in such a case, produce mortification of the tonsils and nearly
nearly destroy the patient. This at once proved to his satisfaction that it was not syphilitic and could not be.

Such is the history of the treatment of syphilis, a matter of the venereal disease for some three hundred years, and such a feeble picture of the results too often produced. Let us follow its history down to our own times and compare the present with the past. The first great step in advance was made in 1810 by Mathias who declared that what he calls the mercurial disease might be induced in syphilis and he speaks of mercury changing the pains, sores and ulcerations in the body into pains and ulcerations of its own specific nature, exciting new excoriations, eruptions and ulcers on the parts with pains, nodes and various other affections of the tendons, periosteum and bones. Still he speaks of mercury as the only panacea for syphilis, saying that those cases alone are hopeless where from emaciation it cannot be given. Differing in this, however, from most that alive to its deleterious influences and to the consequences that it was capable of producing, he urges its immediate cessation on the appearance of any of those symptoms described, and declares emphatically that its third continuance is death to the patient. He urges also the giving of it in much smaller doses and for a much shorter time.
stating that when given long in excessive doses it appeared
to destroy the energy of the nervous system, causing
weakness, tremors, polyneuritis, epilepsy and mania
(in addition we suppose to the special evils above men-
tioned). Still cognizant of all these evils and believing,
as he seems to have done, that when given in venereal
disease it produced these fearful results assigned formerly
to the venereal poison itself and though he says that
most of the fatal and supposed incurable cases of lues
Venera would prove to be instances of the mercurial
disease, it never seems to have occurred to him that
syphilis could be cured without mercury. It certainly
was a great advance when the effects of mercury were
ascertained to mercury itself and its exhibition forbidden
when these were manifested, but a greater had still to be
made. In 1812 Dr. Ferguson, Inspector General of
hospitals to the Portuguese Army, in a paper read before the
Medics Chirurgical Society mentioned with considerable
surprise that in Portugal mercury was not used in
syphilis and yet that the Portuguese victims of it re-
covered in a manner contrasting much with its
terrible ravages amongst our men (who of course were
most carefully mercurialized). He says of the British
army it is probable that more men have sustained
the most melancholy of all mutilations during
the four years that it has been in Portugal through
this disease than the registers of all the hospitals in
England could produce for the last century. While
Venernal ulceration has not only been more intractable to the operation of mercury than under similar circumstances at home, but the constitution while strongly under the influence of this remedy has become affected with the secondary symptoms in a proportion that could not have been expected. With the natives, on the contrary, the disease was very mild, curable for the most part by topical treatment alone, or wearing itself out when received into the constitution after running a certain course (not always a very destructive one) without the use of any adequate mercurial remedy. Again, he says, they (the Portuguese) go out of hospital cured by topical means alone, and their return for secondary symptoms under such circumstances is far from unusual or even frequent. Here, then, was Syphilis cured without mercury, as Ferguson evidently a firm believer in that drug is forced to admit, but then the idea of its specific power still reigned in his mind. If the disease could be cured without mercury, it could not be true syphilis such as is seen in Great Britain. There must be some difference, and to explain this he does not lay the blame of its more terrible character in his own country on the mercury used to cure it, but brings forward a most remarkable theory, that in Portugal the disease being inadequately opposed (i.e. mercury not being used) it had exhausted itself. A most remarkable theory certainly it is. A fire burns itself out when there is no fresh fuel supplied and a disease has exhausted itself when it has attacked.
attached all within its reach. His idea seems to be that
through universal prevalence for ages, a sort of immunity
had been obtained, or at least, a mild kind of Syphilis
induced, but if such be his idea, he somewhat mars it by
stating that he has seen the Portuguese soldiers turn out to
duty with ulcers that made him shudder, and that
does not look as if the disease had exhausted itself or
even as if much immunity had been gained from it. But
then the secondary symptoms in general were very mild
and there, we suppose, the good effects of the universal
prevalence of the disease became manifest. Such, then,
was it amongst the Portuguese, but different far with our
men who had the good (or) fortune to be under the care of men
who sneered at the Portuguese physicians for their igno-
rance in treating this very same disease. It seems truly,
to have prevailed to a terrible extent amongst our troops
as may be seen from the passage already quoted, but of
course its ravages could not be due to Mercury which these
ignorant native practitioners did not use and which
they looked upon and considered with horror as a poison
which foreigners madly used. It could not be it, there-
fore it must be due to this new virus, exhausted though it
was, being unfriendly to strangers. "To change of climate,
modal of life-to anything but Mercury which alone," he
says, "can combat its terrible effect." This means which
the natives employ to cure themselves, being utterly unable
to combat it, and he says to pretend to treat it with
vegetable remedies would be as idle as to trust the care of
acute
Acute pneumonia to ptisana or hepatitis to barley water. Now we cannot help thinking that with such facts as were before him Dr. Ferguson might have, at least, tried the Portuguese system before he declared it useless. A thing which he seems never to have thought of as wedded was he to his prejudices in favour of mercury. Had he tried it, he would probably have found what has since been found in reference to ptisana in pneumonia, that it was a system better far than that he advocated and that its merits depended not on the degeneracy of the virus but on the withholding of mercury and perhaps his language would then have been more civil in reference to some German surgeons who refused even officially to give mercury which he characterizes as most alarming. Conduct referable only to the most brutal ignorance. Ferguson then did nothing to further the treatment of syphilis except by publishing the mere fact that in Portugal it was cured without mercury. He evidently believed in mercury and in it alone being able to combat that disease in the British subject. But in 1817 we find another army surgeon Dr. W. Rose going far in advance of anything that Ferguson had even dreamt of, and actually daring to treat syphilis and that British syphilis without mercury. He seems to have been led to attempt this by several considerations, and amongst others by Ferguson's paper. Holding the opinion as to the degeneracy of the virus to be a mere opinion inconsistent with known facts
facts and with his own experience among the Spanish
and Portuguese and wisely resolving to try, before he
condemned, he did try and succeeded. Which had done
so before him even in this country but to him certainly
belongs as small share of the honour that accrues
from the great change in treatment and the equally
great and good results that have flowed from it.
It required no small courage in him to bear thus
bravely the opinions and practices of three Centuries
to hear the Contumely that such a course was sure to
draw upon him, and in the face of the treatment of
the German Surgeons already mentioned, boldly to
declare that Syphilis could be cured without Mercury.
His paper is peculiarly valuable because at the time
a firm disciple of the old school he was very peculiar
not to give up Mercury, but at length satisfied from his
own experience that some cases at least could be cured
without it he began some experiments in his own
regiment and finding that very short courses of Mer-
cury and such as would be regarded as utterly in-
adequate succeeded in curing, he laid it aside
altogether and he acknowledges that the result surprised
himself and must have surprised many besides. He found that during an experience of nearly two years in
a regiment 1000 strong stationed in the Metropolis (London
Bilharz is affirmed by Bock to be the most populous in the
world) in every instance a cure was effected without Mercury
at all and when secondary symptoms did manifest
themselves
themselves they were in all cases slight. So that he says, 
"Era of the bones and some of the most equivocal symptoms 
did not occur, and in no instance was there that uniform 
progress with unrelenting fury from one order of symptoms and 
parts affected to another which is considered an essential of the 
Syphilis", even each individual symptom of which has 
been declared by some to be regularly progressing and never 
to be checked except by mercury. Here, then, we have it distinctly 
laid down that Syphilis can be cured without mercury and 
we find this view supported in this paper by numerous 
well-marked and well-authenticated cases. This idea was 
soon taken up by others and by some more strongly than 
by Prof. John Thomson of the Edinburgh University, to 
whom may be attributed the purity of the use of mercury 
in Syphilis and the good results flowing therefrom in 
Scotland. That these results were good, we have some 
experiments on a most gigantic scale to prove. Thus in 1822 
we have the matter tested in Sweden and the results of 6,000 
cases treated in the military hospitals of that kingdom, one 
half with mercury, the other without, are as follows — 
In those treated with mercury the relapses are 17% in 
100; in those treated with it the relapses are 18% in the 
same number. In 1828 Fricke published the results 
of his experiments in the Hamburg Hospital. In 
1549 cases of both sexes, 58% were treated by a mild 
mercurial course and 1067 without mercury, the 
mean duration of the latter treatment he found to 
be 51 days, of the former 85. He found also that 
relapses
relapses were more frequent and secondary syphilis more severe when mercury had been given. He maintains that he has treated more than 5,000 cases without mercury and has still to seek a case in which that remedy may be useful. He has never observed caries, loss of hair or pains in the bones follow his treatment and in all such cases that have come under his notice much mercury had been given. In 1833 we have the report of the French Council of Health and the result seems to be that Cases treated with mercury are one third longer in recovering than those treated without. At Straubing between 1831 and 1834 5271 cases had been treated, mercury being given in only the most obstinate and the number of relapses and secondary affections was very small. No case of caries and only one or two of tecostosis occurred, whether in those treated with or without mercury is not specified. Since then innumerable cases have been treated without mercury with results as successful perhaps even more so. Such then is a short resume of the history of the treatment of syphilis and we think a consideration of it fully justifies the following conclusions:

1st. That mercury has been enormously abused in the treatment of syphilis and that that in one or two cases but as the regular practice for some 300 years. The amount of injury and suffering inflicted on humanity by this abuse we can form but a slight idea.
idea of, even from the records of the results that
have reached us: but it must have been terrible.
No wonder syphilis was dreaded, for truly it entailed
severe suffering, not in itself so much as in its
treatment, that which was given to have too often
destroying.
3. That as the use of mercury declined syphilis became
less severe and less terrible. We cannot doubt that
it was less formidable malady under Hunter's
treatment (bad though that was) than under Wilmann's
or that it was less severe under the treatment of
Mathias than under that of Hunter. The terrible
ulcerations, diseases of bones and hideous deformities
became less and less frequent as less mercury was given
till as we have seen those never saw them at all
nor have they been seen in the many thousand Cases
already referred to and consequently,
3. Mercury is not a specific for syphilis and not
withstanding the great names that have advocated
it never was so, and to it are the ascribed in most
Cases the evil results that followed its use, long
wrongly attributed to the syphilitic poison and
which to almost an incredible extent disappeared
with the prolonged and excessive use of the mineral.
4. Mercury is not necessary to the cure of syphilis,
primary, secondary or tertiary, even in minute doses
thousands of Cases, as we have already shown, being
recorded where mercury was never given but which
perfectly recovered.

5th. That when no mercury is given the occurrence of secondary Syphilis is more rare, and if it do occur much milder in character than when that drug has been given.

6th. That all theories, however plausible, based on the antisyphilitic power of mercury are erroneous and false, its history proving that for from neutralizing the venereal poison by chemical union as some maintain, it rather forms with it a new and more tenable compound, and that instead of (as Hunter supposed) displacing the action of the venereal virus by one more manageable, it rather unites its own power to that of the other; instead of the one fire putting out the other burning, the two fires seem to combine to destroy the patient. In proof of this we have only to compare Sydenham's results with those of Wolfe.

These considerations follow naturally from the facts adduced and show that even in the present day mercury is abused in being used so often as it is. In hospital practice how many diseases are not traced to the use of mercury in Syphilis, which are never seen unless that drug has been given, and every now and again cases are seen illustrating the practice of our predecessors in a manner anything but creditable to the profession. Thus in T. Bennett's work on the Principles and Practice of Medicine I find the following case, by no means a solitary one, sufficient in itself, we would think, to cause mercury to be discarded in the treatment of Syphilis. Anne Bruce, Oct. 24,
admitted Jan 10th 1852. Her face presented a most fearful appearance, being covered as well as the neck and upper part of the cheek with circular masses of pustular scabs varying in size from a fourpenny piece to half a crown, several being in some places crowded together. Some of the prominent scabs were dry, others soft with foetid pus oozing from their bases. In some places they had fallen off exposing circular, unhealthy looking ulcers. Whenevver the skin could be seen it was of a fiery red colour and pockmarked with old cicatrizes. The lower lip was swollen and the left lower eyelid was ulcerated and everted. The metacarpal bones of the left hand were enlarged and the skin covering them red and painful. External appearance highly suicidal. Five years she contracted primary sores from her husband, followed there by a minute pustular eruption on the skin, which shortly disappeared but was succeeded by occasional blotches which sometimes broke but always went away slowly. Eighteen months after the commencement of the disease she came to Edinburgh being alarmed for the appearance of one of the blotches on her chin. The practitioner in Edinburgh placed her under a mercurial course and she was salivated for six weeks. The disease on the face instead of healing slowly as before now ulcerated and began to spread. Six months after she was again salivated for four weeks but the whole lower half of the face was involved and she entered the Royal Infirmary. There she nearly recovered under the use of iodide of potassium and topical emollient applications.
Six weeks after being exposed to cold and wet, the blisters, scabs and ulcers returned on the face and gradually spread to the neck and chest as formerly described.

This case seems to me a terrible commentary on the theory that mercury is useless in Syphilis. We have no doubt that occurring in Edinburgh, the salivation, though long sustained, was gentle, yet what a terrible result? for Can we impute the result to anything but Mercury or its combination with the prevalent virus? Many told that it is useful in chronic cases, but if by its use we run the risk of results so terrible, better let the Syphilis take its own time of departing. I believe that in Syphilis we have the same indications for treatment as in smallpox or fever. There is a specific virus in the system producing certain results; we cannot destroy that poison where it has once entered the system, nor can we prevent its specific effects; but by careful dieting, rest and emollient local applications we can modify and direct, and thus we believe all or nearly all cases may be cured utterly, independent of mercury, which we believe, has no more specific power over Syphilis than over smallpox or Syphilitic fever - diseases which physicians now never think of claiming the power to cure by any drug whatever, being content to stand by and direct, modifying and controlling but never trying to abort. And such we think, should be the physician's duty, when the Syphilitic virus has entered the system, we believe.
believe that by using the same means in this disease as in the two others mentioned, syphilis would become still less formidable than it is at present, and that such cases as the one detailed would never be seen in our streets or hospital. We know well enough that the syphilitic virus may of itself produce serious results. Thus primarily extensive and severe local ulceration and inflammation may be excited; and failing local erosions, which if seen in time should always be attempted, secondary symptoms sufficiently severe may follow, as even tertiary, but we believe that these are rare under ordinary precautions and would be far less were proper hygienic means adopted from the first as in small pox or fever. Lay the patient up in bed, promote the evaporation of the virus by diaphoretics and diuretics; regulate carefully the diet and, we believe, syphilis will cure itself and will prove itself to be a self-limited disease. But depress the system by medicine or thwart by exposure or other means the due elimination and you have consequences more or less serious following according to the circumstances. Now mercury seems in many cases either to retard elimination or to form with the syphilitic virus a poison more terrible than the original and only to the precautions as to diet and regimen necessitated by its exhibition and probably else to syphilis being self-limited has mercury owed its reputation. Certainly its least title to be called an antisyphilitic, as we think its history has sufficiently demonstrated.
concomitant circumstances when successful, we think its history shows, was the success attributable to its exhibition and continuation with the general poison, we think may be traced those terrible results that so long disgraced and ever will disgrace the annals of medicine.
Mercury as an Antiphlogistic.

In almost every acute inflammation we find mercury trusted to as the second means of cure, blood-letting being the first; and it is said, especially when preceded by resection, to have some peculiar power over the inflammation, so great that it has been held as amongst the first of our so-called antiphlogistics. Its use in inflammation is of but a recent date compared to its use in Syphilis and in hepatic diseases; but it has been quite long enough to enable us to judge candidly of its antiphlogistic power; and I think it can be proved that inflammation can be combated as successfully without as with it. In discussing this question we shall consider the grounds or theories on which it is given, and see how far experience confirms or nullifies the assertion as to its alleged effect on inflammation.

And it is supposed by many that in inflammation, mercury given so as to produce febrileism prevents or limits exudation and at the same time hastens the removal of what has been exuded. It is also said to oppose the inflammatory process by dissolving the Corpuscles and removing the abnormal proportion of fibrin found in the blood in cases of inflammation; and from possessing or being supposed to possess these powers, it has been called a specific in inflammation. Now it is not very difficult to prove that it does not at all events prevent exudation.
exudation completely or totally in all cases, for many might be adduced in which exudation has been known, but while the system was under the full influence of the mineral. Thus Dr. Taylor gives four cases of pneumonia supervening after the full establishment of ptyalism. Dr. Bennett tried it in a case of pleurisy of one side and even whilst under its influence pleurisy of the opposite side developed itself, and many cases of a similar nature might be given. As to its budding or controlling the exudation of coagulable lymph, we cannot see how it can be proved to possess such a power. We cannot see how in any case it can be said to have prevented or limited the exudation, for we know that in many cases complete resolution may and does take place without mercury being given at all, whilst in others the exudation thins out is very minute in quantity, and these facts ought to render us suspicious of the effects being due to our remedy in any given case. Alix so, indeed, explicitly says, the cases in which that combination (Calomel and Opium) has seemed most useful, have been those most frequently in which the symptoms having subsided, it was withdrawn without the mouth being touched, and therefore without any proof of its specific virtue. And Abbevin maintains that he has seen the effusion of plastic lymph increase during its action and says that in acute pleurisy he has seen the effusion go on increasing as long as mercury was given and unless discontinued it would
it would probably have destroyed the patient.
So it seems that its power of preventing, stopping and controlling exudation is founded on very hypothetical
grounds indeed, and may well be disputed. But it
is said to hasten the absorption of the plasma exuded.

A fact, we think, also very difficult to prove, as we can
never, or seldom ascertain how much the disappearance
of the exudation depends on our remedy; but appar-
ently strong facts have been adduced in support of
this action of mercury and none stronger than the
evidence of our own eyes in duties, which is always
pointed to as the epee signum by those who advocate
mercurial treatment. Thus Watson says: "Bleeding
and purging may have failed to stop the mischief
going on in the eye and the deposition of lymph; in-
creasing, but the instant that the gums and breath
acknowledge the specific agency of mercury upon the
system, the red zone surrounding the cornea begins
to fade; the drops of lymph to lessen, the Iris to
resume its proper tint and the puckered and ir-
regular pupil once more to approach to the perfect
circle, till at length the eye is restored to its original
integrity and beauty and usefulness." Certainly no
language could be stronger, no action seemingly more
perfect, but unfortunately it seems to be a mere type
-a sort of beau ideal case for the Continues. I speak
only of favourable cases, the changes described some-
times take place slowly," and he speaks of the
Mercury
mercury sometimes requiring a month or two to perform a cure. Now we know, and we object to the fact here attempted to be proved, 1st that Sitis of the most severe description has been cured without mercury at all being given, as we find in a most severe and interesting case recorded by Dr. Bennett, and Dr. H. W. Williams brings forward 64 Cases of every grade of severity and of all kinds treated without a single grain of mercury, yet with results perfectly satisfactory, perhaps more satisfactory indeed than statistics of the mercurial treatment could give. This winter too we have had the opportunity of studying a similar case in the Clinical Ward under Dr. Bennett, not quite so severe as the one recorded, but still very well marked and nothing could be more thoroughly satisfactory than the result. From these facts we argue that the disappearance of the disease, when mercury has been given, does not prove that mercury cured. 2d We know and even the most ardent advocates of mercury have been forced to admit, that exudation in the eye is often very speedily absorbed after the subsidence of the inflammation without any mercury having been given. Thus in Common Conjunctivities, today the whole conjunctiva may seem red and injected with blood, tomorrow there may be no or scarcely any traces left of an action that seemed intense and threatening. Thus in Sitis itself the exudation has often quickly disappeared when no mercury has been given. Thus Lawrence states that
in one case treated by him without mercury, the exudation was absorbed as soon as the inflammatory ceased, and that he never saw it disappear more quickly under any circumstances whatever. And from this we argue that the disappearance of exuded plasma after the exhibition of mercury does not necessarily prove that the mercury caused or hastened its absorption. 3rd. We know that the eye has been lost even when mercury was most carefully given. It then failing to cut that the exudation, or by hastening absorption to prevent the higher and more serious results of the inflammatory process. 4th. Mercury has often failed completely to cause absorption in the eye and elsewhere. Thus in the case of pleurisy above referred to Dr. Bennett speaking of the result of the exhibition of mercury says, ‘Salivation was maintained for ten days; at this time such was the discomfort it produced, so thoroughly did it destroy the appetite and diminish the vital powers that it was discontinued. Yet so far from causing absorption it not only failed to do so, but under its influence pleurisy of the opposite side supervened.’ Such cases are by no means rare and abundance could be adduced, where it has failed completely to remove solid or fluid exudation; and Dr. Aspin says, ‘In pneumonia no reliance can be placed upon mercury in preventing or resolving the putrefaction of the lungs.’

These considerations seem to me sufficient grounds
grounds for doubting the alleged power of mercury in hastening absorption, or at least for maintaining that its action, as observed in many cases, does not prove it to have such a power. But many, while doubting the action of mercury in promoting and accelerating absorption, maintain that it can and does arrest inflammation and the deposit of coagulable lymph. Thus Lawrence says, "fulsalivation quickly produced cut its short recent diseases as if by a charm." Watson says, "Mercury clearly has the power of arresting the deposition of lymph and of putting an end to the adhesive inflammation." How to this we object, that mercury often fails to cut short recent diseases. Nay, that it sometimes aggravates them. Thus Taylor gives several cases of apparent aggravation of pericarditis through the induction of salivation, as well as other cases where the action or influence of the mercury on the system did not prevent, if it did not cause new complications—where instead of curing the disease for which it was given, it could be reasonably enough inferred it had acted as the exciting cause of others. Certainly it does not cut short all acute diseases—it does not arrest all adhesive inflammations, and just as certainly many acute diseases he said to be cut short in many cases by other remedies and adhesive inflammation to have been arrested. Thus take a case (and I describe a case that I saw treated in the hospital this winter). A man was seized
seized with acute peritonitis of a very severe character.
Here was an instance of adhesive inflammation of a
most acute nature. Hot fomentations were ordered to
the abdomen, and opium in frequent doses, but no
mercury. On the 11th day the man was quite Convales-
ccent. Now I think it impossible that a disease
of such an acute nature as the one referred to could
be cut short in less time than it was. Whether was
it the opium or the hot fomentations that arrested that
adhesive inflammation - that cut short that recent
and certainly very acute disease? Had mercury been
given, as under the old regime it would, to it the
result would most undoubtedly have been attributed,
but it was not given and what there, the inference?
Simply that it is very difficult indeed to say in any
case that any remedy has cured a disease and
that owing to this circumstance drugs may have
false virtues ascribed to them. I could easily adduce
cases of all kinds of inflammations as much cut-
short by other treatment as they could ever be proved
to be by the mercurial - Cases where hot fomentations
or a dose of opium acted as much like a charm as
ever did Colonel or other preparations of mercury.
May I think it can be and has been proved that
some acute diseases are now more frequently cut
short or at least, less frequently terminate fatally,
since the two great Antiphlogisties (blood-letting
and mercury) have been discarded from their treatment.
I need only refer to pneumonia to establish this; and certainly the statistics of that disease do not prove mercury a very active agent in arresting acute diseases in preventing and controlling exudation, or in causing its absorption when thrown out, but in my opinion go far to prove that it does not possess any such power at all, and that it has owed its reputation in great part to the fact that the inflammatory process may spontaneously cease, complete resolution taking place (a fact too much overlooked in therapeutics). As also to the careful regulation of diet and hygienic during a mercurial course. We now know at any rate that diseases in which mercury was once held and maintained to be "miracle" can be cured fully as well without as with it. We have already mentioned pleurisy, pneumonitis and peritonitis and we might adduce many more; in fact, I believe, we might find examples of every disease recovering as well without as with it, so as Haberlandt declines better in most cases. He maintains that the exhibition of mercury in inflammation of the brain and lungs and of the serous membranes as the pleura, pericardium and peritoneum (in which it was once thought most useful) is positively injurious, and he supports this opinion with some well chosen cases. These facts speak for themselves, and surely give us pretty good grounds for maintaining that the concomitant circumstances had more to do with the acquiring of its antiphlogistic reputation.
reputation than any such power possessed by mercury itself. At all events the proof that it can prevent and control exudation, that it can hasten absorption through stimulating the absorbents, cut short recent acute diseases and arrest adhesive inflammations, seems to me extremely defective and far from satisfactory.

But it has been maintained that mercury opacifies inflammation through dissolving the Corpuscles and removing the abnormal proportion of fibrine found in the blood, or according to Viridia is a *psammatic*. Now this view, I believe, is founded on a false pathology and it may be disproved; 1st. By proving that the solution of the Corpuscles only tends to increase the fibrine; And 2d. That the increased quantity of fibrine is not the cause but the consequence of the inflammation. 1st The fibrine is not effete matter but is in reality the plasma out of which the tissues are nourished and it is ever renewed and renewed by the solution of the Corpuscles which thus subserve a most important function. This is proved by the following fact. Majendie's experiments proved that if blood was withdrawn, defibrinated and again replaced, and this repeated sufficiently often, the animal died of starvation (the fibrine being the only constituent withdrawn and food being given). Again the fibrine of the blood differs in chemical constitution from that of flesh and so cannot be effete flesh. Again the plastic exudation is spontaneously Coagulable
Coagulable and fibrin is the only element of the blood that is co. These and other considerations prove it the source and basis of all growth, but it exists in but small quantity in the blood and the question is how is that quantity kept up. We believe as we said before by solution of the Corpuscles which do contain fibrin and which we may look on as the elaboration of that fibrin. This is proved by the fact that when the Corpuscles are diminished the fibrin is increased in proportion and vice versa. Now in inflammation the fibrin is increased, that is today, there is more actual fibrin floating in the liquor balsaminic than in health and the Corpuscles are diminished in proportion. The Corpuscles decreasing as the fibrin increases. So that the fibrin in the blood is not actually increased in quantity in inflammation, but has only changed its place, having passed in greater quantity from the Corpuscles to the liquor balsaminic. Now from this it is quite evident that mercury by increasing the solution of the Corpuscles would increase the amount of fibrin; as its opposing inflammation or at least increase of fibrin by dissolving the Corpuscles. I think, we may discard and consider now, granting that it does diminish the proportion of the fibrin, if it thereby opposes inflammation. Milder and Williams maintain that the increase of fibrin (as oxides of protein) causes the inflammation. But Andre's experiments prove that the increase is not the result of inflammation, proving that the inflammation...
inflammation precedes the increase of the fibrine and disappears as the fibrine diminishes. The fact seems to be that inflammatory action taking place in a part, weakens that part, but the system at large is stimulated and its vital activity increased, and through that increased vital activity increased solution of corpuscles causing increase of fibrine takes place not in the part affected, where vitality is lowered, but throughout the system at large, where the vital processes are increased.

If this is the fact, then that the increase of fibrine is the result, not the cause, of the inflammation, what good does its diminution accomplish? It is no morbid matter—no poison, it does not cause the pyrexia and though it constitutes its excess does not cause the plastic exudation. By simply diminishing the fibrine you never would remove the producing cause. But does mercury actually diminish the amount of fibrine? We have no proof that it does. We know that blood-letting even must be carried to a great extent before it can diminish the fibrine and only when it diminishes the inflammation producing the increase has it any effect at all on the proportions of fibrine, and we see no reason for believing that mercury is at all more powerful in that respect. We know that under the action of mercury patients get thin and anaemic and Dr. Farre maintains that it quickly destroys red blood, giving where in six weeks mercury blanched a complexion compounded of the red and
and the violet, and made it as white as the lily. But perhaps a good deal of that might be attributed to the loss of appetite and imperfect nutrition consequent on the use of mercury. We have no doubt that six weeks' abstinence or six weeks' purging by any medicine would do the same. Granting, however, that mercury can render a patient pale, thin, emaciated and anaemic it does not follow that it diminishes the amount of fibrine; for we know that even in extreme anaemia, as seen sometimes in Confirmed Chlorosis, the fibrine is even higher than normal. The corpuscles being diminished and in proportion to the increase in the one is the diminution in the other. So that whether the production of such a state in inflammation be desirable (which we believe it is not) it certainly fails in its end if that end be the diminution of the fibrine of the blood. In fact I think it very doubtful, if any medicine that we have, can diminish the amount of fibrine. Salines have been said to possess that power, but we must distinguish between diminishing the coagulability and diminishing the actual amount of fibrine. At all events as we have already shown, even though mercury could reduce the proportion of fibrine, it did not therefore cure inflammation; for the increase of fibrine is not the cause but the consequence of that inflammation.

Mercury, again, has been said to supposed to cure inflammations by equalizing the circulation and thus
Oriating congestion in any one part. Now it is thought
this might be disproved by the simple fact of mercury
being, as it is, a dilator and increasing, as it does,
the secretion of saliva so much, for we cannot believe
that it increases the other secretions in any thing like
the same proportion. Indeed it seems doubtful if it does
at all increase some of them (as we have already shown
in reference to the liver) and closets thought he proved
by experiment that it diminished the vascularity of the
gastro-intestinal membrane. Be that as it may however,
no one I think will maintain that the other secreting
glands are stimulated to any thing like the same extent
as the salivary. Consequently the determination of blood
to these glands must be greater. They must be in a
state of hyperaemia, which shows at any rate that
mercury does not equalize the whole circulation in
reference to all the organs. Had it been said that through
causing determination of blood to the salivary glands
it acted as a deravit, though certainly its supposed
power would not have been explained, the hypothesis
at any rate would have been feasible. But even its
derivative action being granted, I cannot see that
on that ground it can be held to be more powerful
in inflammation than say drastic purgatives, which
probably cause greater exhalation from the blood and
produce greater temporary congestion without exposing
to the dangers incident to mercury. But it is rather
strange, if it does possess this derivative action, that we
find
find in the very cases in which it might be expected to do much good, it actually does harm. Thus Naberhaus maintains and gives cases to prove that in inflammation of the brain, lungs, pleura, pericardium and peritoneum, whilst in some cases the products of disease become absorbed and health restored, in very many instances it leads to renewed effusion. Taylor and Bennett's cases confirm this. Swan attempted to prove that mercury acted chiefly through the sympathetic system of nerves, but he assumed what we now know to be false viz., that mercury was not absorbed and never found in the blood or excretions, and his experiments I think fail to prove his theory, though evidently undertaken to prove a pre-conceived idea.

We have thus endeavored to show that the hypotheses adduced as to the action of mercury in inflammations are, to say the least, doubtful. We have shown that many inflammations the most acute can be cured without mercury and that many have seemed to be aggravated by its use, whilst it has often failed to prevent inflammation in a system under its full influence. And we think we may safely conclude that it has been used too empirically in inflammations and has obtained a reputation which it does not deserve. We see no reason for the many high Eulogiums passed on it as an antiphlogistic—no ground for the assertions that in inflammation it is our shield ancl as above all no foundation for its much affirmed specific power.
power. In our textbooks we still find it recommended as the most trustworthy of all our agents in the treatment of disease. Like Watson for example, and we find its exhibition recommended in almost every disease: in pericarditis, in rheumatic fever, in pneumonia, in peritonitis, in pleurisy, et cetera omne genus. We are told that on it, after resection, we must trust; yet we find all these cured as far as they can be cured at any rate, at least as speedily and as thoroughly without as with it, and if so much more safely. There is a great deal of diseases, as the most important, capable of being caused by mercury: none of them are trifling, many of them most serious and terrible. Idiosyncrasies in reference to this medicine are not uncommon, and we find not a few cases scattered through the records of medicine, where mercury given in very small doses even has caused hideous deformity and death. If the risk of these can be avoided, surely it ought—of other means in general succeed as well, surely these other means should be tried, and if they do succeed as well as a great proportion of cases, then let us strip mercury of its false laurels and discard it from the treatment of inflammation and the sooner the better. The belief in its specific power in disease has we believe, done much to retard the true progress of medicine. It is a comfortable word to fall back upon when we give anything empirically, but it frets, research and prejudices against true observation and generalization.
Conclusion

Thus, then, have we endeavoured to ascertain the truth in reference to the action of mercury as a Chologogue, an Antisyphilitic and Antiphlogistic (for of these actions alone were we treating) and we think that, however imperfect our discussion of the subject may be, it at least establishes the fact that mercury is not a specific in Hepatic disease, in Syphilis or in inflammation, and that it has been given too profusely and consequently has caused much suffering and even death, that all these diseases have been treated as well and even indubitably in many cases much better without than with it, and that the present practice of those who discard it altogether in the treatment of the diseases above specified, is founded on good and satisfactory grounds. We do not deny and it would be difficult in the face of the many cases that are recorded, cases too numerous to be mere coincidences, to deny that mercury is a curative agent of some value, but we believe it owes its value as such not to its Chologogue, its Antisyphilitic or its Antiphlogistic powers, but to its power as an Alterative (whatever may be meant by that term), a power which we believe mercury does possess and which renders it valuable in certain cases, especially of a Chronic nature, but which it is not our province to discuss.

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